

18

OUTLINES
OF
MEDICAL PROOF.

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THE able Hunterian oration, given by Mr. Green in the spring of last year, leaves little to be desired in regard to the object which it has in view, at least so far as amplitude of language and conception is concerned. With his particular views on the subject which he thus amply discusses, I am not concerned; for instance, I neither question nor affirm his doctrine on the nature of genius. But another branch of his great subject, which may be called the objective branch of it, remains untouched; and I trust he will undertake and exhaust it. I do not propose to occupy the ground, which belongs peculiarly to him; but to throw out such views in reference to its confines, divisions, and extent, as may tempt him to seize and occupy it. My illustrations will be drawn from my branch of our common profession; but the principles on which I proceed, if they are founded on truth, must apply to both.

The immediate application of the mind, cultivated as I shall presume it to have been, on the principles laid down by Mr. Green, is beset by many difficulties in

respect to the mixed and varied modes of reasoning which are called out by this complicated subject. "The analytic and synthetie functions may," as Mr. Green observes, "have been exercised in the grammatical studies of the pupil, but henceforth they must appear in their proper character, and be practised with conscious ability." But, in order that the aspirant may thus fulfil the great task before him, he must have been taught, or must teach himself, to form a just conception of the peculiarities of proof incident to his peculiar subject, and its auxiliary branches of knowledge. The remarks in the following pages may help him on his way towards this object.

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OUTLINES

OF

MEDICAL PROOF.

MEDICAL PROOF, or the proof which we obtain from various sources, in reference to the practice of medicine, must arise from mere observation, or from experimental observation, or, I may say, for convenience, from observation or experiment.

Observation operates either on subject matter, placed so far at our disposal that we can modify and alter it, or on subject matter, the phenomena of which we can collect and register, but cannot alter. Of the latter kind, the observations of the astronomer are instances. Those of the physician are of both kinds; but the former kind of observations are most subservient to his purposes.

We are said to obtain proof by experiment, when we not only can alter and vary our subject matter, but also are enabled to investigate its properties “*per exclusiones et rejectiones.*” * This “*separatio naturæ*”—

* *Novum Organ. lib. i. aphorism. cv.*

I use the language of its great enunciator—duly carried out through a sufficient number of affirmative instances, enables us to draw confident conclusions; while observation, unallied to experiments, suggests probabilities of more or less strength.

When the physiologist observes the sequence of certain movements or certain modes of excitement, if, on frequently renewing his observations, he each time sees them in the same relations, that law of our belief, which disposes us to expect uniformity, begins to operate in his mind, and the more frequently the observation is repeated with the same result, the greater his confidence becomes as to its being a natural coincidence. Such was the belief of Prochaska respecting certain functions of the nervous system, now called the reflex functions; and this was a probability obtained by observation. But the experiments originated by Sir Charles Bell superinduced that certainty, which belongs to their peculiar evidence, on the above conjecture, by demonstrating the existence of the double roots of certain nerves, “efferent” and “afferent.”

An experimental induction assigns causes, that is to say, necessary antecedents to phenomena, by excluding all that are incidental. Once made, it is complete. No induction of additional instances is asked for with a view to the conclusiveness of Dr. Wells's theory of dew. On the other hand, in the induction from mere observation, every new instance has a value in bringing us nearer to certainty.

Now, however desirable the certainty produced by an experimental proof, it must be admitted that the

method of observation is the main source of those principles which govern the practice of medicine. The very circumstance of vitality and its mysterious laws being in great part the subject matter of our reasoning, accounts for this. It is difficult to obtain such a command over this subject as to interrogate nature through the rejections and exclusions of experiment. The organism is liable to be deranged by the measures requisite to carry out the inquiry. Still we know, that this difficulty is occasionally surmounted by well arranged measures, as in the discovery above alluded to. The “*mera palpatio*,” of unmeaning and casual experiment, of course does not enable us to assume the credit of this kind of proof.

The ultimate application to practice of our interrogations of nature, whether these interrogations are made through experiment or observation, must be a process of the latter kind. However the inquiry may have begun, there it will end.

The collation of instances having common points, which furnishes the proof by observation, conveys to the mind the idea of a general truth. To this we give an admission proportionate to the number of the instances adduced or the probability of the hypothesis with which the inquiry has commenced, one or both. When this hypothesis is well grounded on analogy, or suggested by glaring facts, it renders our generalisation safe on a smaller number of instances than would otherwise be required. It affords, indeed, an additional source of evidence, that, namely, of antecedent probability. Still it must be admitted, that

some of our most important generalisations have been effected by the patient inductions of observed phenomena, without any aid from preeonceived hypothesis.

A few words are desirable on that argument by analogy, to which I have adverted. This, it must be remembered, is only valuable as a source of proof, when there is truly a point in common between the subject matter, from which it is drawn, and that to which it is applied. A fanciful assumption of some common point may produce a brilliant or witty illustration, but nothing will be proved or even rendered more probable. Thus, where Lord Bacon says, that “virtue is like precious odours, most fragrant when they are incensed or crushed,” the mind, pleased with the illustration, simply becomes more retentive of the idea illustrated. When Mr. Herbert Mayo applies Dr. Yelloly’s fact of paralysed sensation occasioning, under certain conditions, an apparent but unreal loss of motor power, to the analogous case of the section of a branch of the fifth pair of nerves disabling to appearance the prehensile powers of the animal operated on, he converts, what before was an assumption,—namely, the unreality of this latter loss of power—into a probability of the highest order*.

I do not mean, by these remarks, to deny the rhetorical value and beauty of such analogies as I have quoted from Lord Bacon; but merely to point out the fallacy of illustration, when similes are taken for proofs.

* Outlines of Physiology, 3d edition, page 261.

These considerations have brought before us an element of proof, on the choice of which much will depend in a process of inductive observation, namely, hypothesis. Let any one who desires to know how far the quality of the hypothesis or conjecture in which an investigation commences may determine its success, reflect on the confidence with which we now generalise the pathology of dropsy, our observations being guided by the experimental induction of Dutrochet, and then turn to the pathology of fever, as deduced from the indirect and direct debility of Brown. The hypothesis of Cullen was, perhaps, equally precarious with that of Brown, but *his* generalisations have a merit independent both of his hypothesis and his conclusions, that, namely, of a truthful enumeration of symptoms obtained from a wide compass of observations. Now it is in the structure of the hypothesis, which may afterwards be made subservient to a series of observations, that we principally recognise the value of experimental reasoning. Though we may not be able to apply an experimental induction immediately to our practice, we may build on it a pile of observations, from which we may command disease. In this respect we are greatly indebted to Dr. George Burrows for his experimental proof of the state of the cerebral circulation with reference to a presumed plenum of that organ under certain conditions, which had been supposed to imply the absence of atmospheric pressure on the cranium. We are enabled to make an immense, though negative, use of these experiments in classifying cerebral disease. For we can now admit local plethora as an element in such a clas-

sification without any of the doubts and scruples which formerly beset us in assigning to it a place or influence. Thus, conformably with the above views, we have experiment furnishing hypothesis, and may expect that hypothesis will suggest that series of observations which results in practical laws.

I have considered, thus far, the process of induction through observation, in respect to its source and mode of origination. It may originate in a conjecture or hypothesis, investing it with more or less probability, or in a few striking facts, enlarged in number by the gradual growth of experience. I have now to notice the uses of the induction thus completed. It may simply assign a law, or it may suggest a cause by offering an explanation, or it may do both.

We have an instance of an inductive process commencing at once from observed facts, without apparently any preconceived hypothesis, and contented with classifying without explaining, in the discovery of the *Morbus Brightii*. "It is now twelve years," says Dr. Bright, in 1827, "since I first observed the altered structure of the kidney in a patient who had died dropsical. It was not, however, till within the last two years, that I had an opportunity of connecting these appearances with any particular symptoms, and since that time I have added several observations." From this beginning a long series of observations proceeds to the establishment of a very important practical division of the causes of dropsy, or its co-existent diseases.*

But *à priori* reasoning,—in other words, the reasoning

* See Note 1, Appendix.

which presumes causes of some sort, and is not contented with classifying,—is always adopted, if possible, as the ground-work of discovery, and often falsely adopted. The immediate discovery of Dr. Jenner was the result of his having had the *good sense* to pay attention to the young woman who came to seek advice, when he was pursuing his professional education at the house of his master at Sudbury, and who, the subject of small-pox being mentioned in her presence, immediately observed, “I cannot take that disease: I have had cow-pox.” The antecedent probability with which Dr. Jenner and others afterwards invested the discovered fact, by finding out that small-pox, in truth, belongs to a great family of disease, pervading extensively the animal creation, pleased and satisfied many minds, who could not take up with the homeliness of mere induction; but it did not assist, nor has it, I believe, assisted since, in the smallest degree the actual verification of the discovery. The casual way in which this non-hypothetical induction commences, and for some time proceeds, may seem, unjustly indeed, to infer less of merit in regard to ingenuity than of good fortune, so far as the *event* of the induction is concerned. To return to Dr. Bright’s discovery, the merit indeed mainly consists in the wisdom which appreciated the importance of obtaining, by any means, a diagnosis between the organic causes of dropsy, the clear perception of certain phenomena made known by autopsy, which many before Dr. Bright must have passed unnoticed, and the perseverance which converted a gradual process of observation

into a great discovery. The affording, through further observations, the explanation of a discovery, must generally rank after it. When the two attainments co-exist in a given individual, they confer on him, in kind, the double merit attributed by Lord Bacon to successful thought, the being "luciferous" and "fruitiferous." The merit of Dr. Johnson's suggestions, as to the tightly packed structure of the kidney under fatty deposit, determining the altered secretion of urine in granular disease, (on the supposition that he makes out his case) claims the first of these epithets. Dr. Bright has himself supplied a valuable instance of explanatory hypothesis, where he accounts for the presence of chorea in pericarditis by the engagement of the phrenic nerve.

Cardiac disease offers, in the present day, two instances of explanatory induction in course of fulfilment: I allude to the hypothesis of bruit in endocarditis, and of rubbing sound in pericarditis. The latter hypothesis serves admirably, indeed, both to identify and to explain the disorder. It possesses a high degree of probability, both in relation to the other phenomena of the disease, and because the *fact itself*, the rubbing sound, seems scarcely susceptible in kind of any other explanation than that which the hypothesis assigns to it. The endocarditic bruit of Dr. Latham is also both useful in assisting diagnosis, and explanatory as to the manner in which the fact is brought about. This criterion, however, does not possess the same amount of probability as that alluded to in pericarditis; for the endocarditic bruit must be susceptible of another expla-

nation, inasmuch as it is present, in certain anæmious cases, without any ground of suspicion of valvular lesion or deposit. Still, this discovery of Dr. Latham, in combination with other symptoms of cardiac disease, is most important.

In speaking of these last criteria as possessing evidence distinct in kind from that of experiment, I may illustrate my meaning by the explanation which Dr. Williams has obtained, by well managed "exclusions and rejections," of the second sound of the heart. We have here, to all appearance, every exclusion observed, through which the production of this sound, by an action of the aortic valves, should be made, not suggestive, but rigorously certain.

The ingenious argument of Dr. Budd in favour of the causation of hepatic abscess through pus absorbed from ulcers in the large intestines, presents another instance of an induction, explanatory as well as collative of facts through observation, as far as it goes. The force to be attached to it must depend upon the extent to which it may be considered to fulfil the conditions above laid down, of antecedent probability of its hypothesis or number of adduced instances. Dr. Annesley's supposition, that in some cases the abscess is consequent to the dysentery; in other cases the dysentery is the mere consequence of disease of the liver, while, in a third order of cases, the disease of the liver and that of the large intestines are coeval or nearly so; is the best exponent of the present state of the question.

Perhaps the best illustration that can be adduced of hypothesis fortified by experimental induction, and

terminating in induction by observation, is one that I have already adverted to in speaking of Proehaska. "The reflection of sensorial into motor impressions, which takes place in the common sensory,"* was indicated by *him*. Some years afterwards, the double roots of the nerves, appropriated to these impressions, were experimentally made out by Sir Charles Bell and his coadjutors. The subject has since been followed up through a series of valuable observations by Dr. Marshall Hall. It is doubtful, indeed, whether these observations have as yet been productive of any discovery in advance of the hypothesis of Proehaska. One of the most curious laws, which Dr. Hall has suggested, namely, that "the abstraction of the influence of the brain upon a limb permits an augmentation of the irritability of its muscles," must be considered only inchoate. Certainly considerable doubt is thrown over it by the researches of Dr. Todd.

It must, indeed, be remembered, that the inductive processes, which I have here represented in their completed states, in the actual history of medicine often occur in a state of virtual dismemberment. The induction of one writer constitutes the hypothesis, on which another writer proceeds through another induction, before any law has been constructed of sufficient generality for practical application. It cannot as yet be said, that the numerous series of collated and arranged and ably discussed facts, for which we are indebted to M. Andral, to M. Louis, to Bouillaud, to Broussais, and to our own Annesley and Abererombie,

* See British and Foreign Med. Quarterly, vol. xxiii. p. 206.

have either by those distinguished men, or by others, been rendered subservient to all the purposes of classification or explanation, to which they ought to prove available.

A large portion indeed of our most valuable, and, I may add, of our most worthless medical literature, consists of inchoate inductions through observation and experiment, of hypotheses having more or less antecedent probability as connected either with facts or with analogies. And of these hypotheses it must be observed, that all which have a basis in facts or in sound analogies have a certain value, provided the author is himself aware how little as well as how much they are worth, and shows that they sit easily upon him. This logical circumspection is nowhere more observable than in the Notes and Reflections of Dr. Holland, in which he throws out suggestively abundant hypotheses, without failing to let us see that he justly appreciates the quantity of proof which he has annexed to them, or may obtain from them.

But there is a kind of suggestive essay writing, not uncommon in our profession, which bears resemblance to the last-named composition, just sufficient to produce an occasional identification of the two kinds. In the works which I now allude to, there is little, or vague, reference to general principles, but much exhibition of a kind of tact, which undoubtedly forms a part of the medical character of mind, but cannot be communicated in this way through the press. It is desirable that the authors in question should exchange their present plan for that of recording distinct cases. Their

remarks, realised as it were by corresponding facts, would then have a specific value, in place of the vague character, half philosophic, half gossiping, which they at present wear.

There are many grades of comparative merit between the processes of induction which I have been considering, and that to which I now proceed : I may term it induction from a gratuitous hypothesis, by which I mean an hypothesis adopted neither from experience nor analogy, and having no other evidence of its intrinsic truth, than that, if true, it will explain or classify the phenomena. The whole induction is here liable to be vitiated by having a fiction for its basis. This vicious form of hypothesis I have already noticed when illustrating the value of hypothesis from its abuse. It began early in the history of the human mind, and maintained its ground through the first age of inductive reasoning. The ancient physiological conjecture that a certain *πνευμα* is transmitted from the lungs to the heart, thence by the aorta into the system, and eventually returned to the lungs by the pulmonary veins, is not more objectionable for its deficiency in real information, than Maximilian Stoll's explanatory hypothesis of that form of rheumatism which it pleases him to call bilious, in persons, "*quibus aeris et biliosa materies maximam partem ex ventriculo resorpta et ad corporis superficiem dilata in vasculorum exhalantium orificiis hœsit, ibique vellicando dolores rheumaticos concitant.*"* A free use of illicit hypotheses has, indeed, up to a late period abundantly vitiated our views.

* Ed. Paris, p. 25.

“Pathologists of a recent date,” says Dr. Watson, in his History of Dropsy, “speak of a want of tone or energy in the absorbing vessels, of the superfluous fluid not being taken up adequately by the enfeebled absorbents, meaning thereby the absorbents properly so called.” Now, the philosophical error of these pathologists has consisted in their taking up with an hypothesis of absorption, not otherwise proved to be true, than as it suited, as far as they had gone, the facts before them. Wanting perceptive proof, their hypothesis was a form of words only, from which, however, they reasoned with as much confidence as if they had the palpable phenomena of heterogeneous attraction before them.

But gratuitous hypothesis has not been always thus obstructive to science; for it must be admitted that the doctrine of final causes has furnished us with more than one successful hypothesis of this kind. There would seem, indeed, to exist a palpable difference between so-called efficient causes and final causes, as to their influence on research. An hypothesis assumed by him, as to the final cause and uses of the valves, led Harvey on to the discovery of the circulation, while an hypothesis assumed on a theory of efficient causes supplied Brown with a division of fever which admitted typhus and enteritis into the same category.

Independently of the scientific value which a gratuitous hypothesis may *thus* occasionally claim, it is in some cases useful, we might almost say essential, as an exponent as it were of certain researches. No definite idea could, indeed, be conveyed by description of the

“*cogitata et visa*” of microscopical physiologists, either to themselves or others, unless, in expressing them, they had assumed a theory of uses and purposes. It is a fortunate as well as a meritorious occurrence when these minute and impalpable inquiries become capable of an hypothesis directly subservient to practical distinctions, such as the “hepatic venous, and portal congestion,” arising out of Mr. Kiernan’s views. But irrespectively of this good fortune, their tendency to afford light and to open out prospects is of no small value. They are luciferous, if not always immediately fructiferous.

Before I quit the subject of hypothesis, I must notice an application of extemporaneous hypothesis to medical reasoning, which demands more consideration than it has obtained. It is this, perhaps, which more than anything else distinguishes an able physician in practice, provided the power be combined with a just appreciation of the value of such hypothesis, and a readiness to abandon it in the face of contravening facts. A capacity and readiness in executing this process, indeed, is sometimes made a source of reproach to us, as practising a merely conjectural art; and sometimes physicians erroneously humour this supposition. Although, in its immediate application, conjectural, the power which I speak of demands an original talent, and again is never successfully carried into practice except by men of acquired knowledge.

The sense in which I have discussed inductive proof through experiment and observation, has been in substance that which Lord Bacon has laid down as characterizing “true induction;” though I am unable entirely

to subscribe to the depreciatory distinction which he makes between this and another method, inferior certainly to the above, but having many claims to attention. Of this latter induction he makes the following remark :—“ *Inductio quæ procedit per enumerationem simplicem res puerilis est, et præcario concludit, et periculo exponitur ab instantiâ contradictoriâ, et plerumque secundum pauciora, quam par est, et ex his tantummodo, quæ præstò sunt, pronunciat.*”* Now I will venture to suggest, that if Lord Bacon’s far-darting eye had reached the present age, he would have seen this “enumeratio simplex” applicable in statistical researches to the most important uses. He wisely, indeed, suggests the defect to which it is most obnoxious, that, namely, of insufficient number of instances; but he would have recognised the fact, that where the inquiry is sufficiently extensive, the danger from “a contradictory instance” is averted or ceases. The existence, indeed, of these contradictory instances, is so far from endangering the argument, that it is implied in its construction, as must be observed in the calculations of our insurance offices. Now from this source many valuable conclusions are obtained in the science of medicine;—the term “puerile,” used by Lord Bacon, being rather applicable to the occasional purpose of the reasoning than to the reasoning itself. Thus, when the subject matter of the enumeration is such as renders definition impossible, the conclusions arrived at can only deceive. Such are many of the medical enumerations of the Registrar-General, where

* Nov. Organon, lib. i., aph. cv.

an average is supposed to have been struck on cases of given disease, *e. g.*, pleuritis, enteritis, asthma, without any evidence being afforded that the registers of these cases were kept by persons who were agreed as to the definition of the terms, or who would make the same application of them to actual instances*.

In the course of Mr. Hutchinson's inquiry into the respiratory functions, and those of Mr. Phillips into scrofula, instances of this procedure are to be found, resulting in the most valuable conclusions.

I have hitherto contemplated medical proof in its own nature; let me now briefly consider the distinctive character of the materials from which it is drawn, as to their respective aptitudes for supplying it.

In respect to the data of our clinical reasoning, we naturally look to semeiology for their earliest and most ready supply. From this source of proof the mind travels onward to presumed structural changes, and endeavours to read them by observation and experiment in the living organism, or by observation in the dead body. But reasonings based on the phenomena of the living structure, have an advantage over such as depend on cadaveric inquiry, in the nature of their results. To explain the living from the dead structure exposes us to the fallacy of extracting conclusions from premises which do not contain them. If such inductions as Dr. Beaumont's sagacity and perseverance effected, operating on the peculiar case of St. Martin, were often in our power, we should soon arrive at high

* See Appendix, Note 1.

degrees of certainty, and escape the imputation to which we are open in our autopsies, of endeavouring to reason out the principles on which the battle of life has been fought, by examining the field of battle and the bodies of the slain. The information obtained from morbid anatomy tends also, it must be confessed, to localise all our conceptions of disease, and so far tempts us to beg the very important question, whether change of structure is the proximate cause of disease, or whether dynamic disease precedes and originates the change of structure. In these remarks I am influenced by no tendency to disparage our most important *present* source of clinical proof, for such morbid anatomy must be considered.

But the researches of the present century are beginning to furnish ample means of reasoning from the living structure, both primarily and in relation to the products of that structure. For this we are indebted to the labours of chemistry; and perhaps in this direction, more than in any other, we may expect medical science to be progressive. Such observations as M. Franz Simon affords us concerning an altered state of the blood in various diseases, and, I may add, under the use of given remedies (*e.g.* the diminution of the quantity of fibrin in a case of phthisis where cod's liver oil had been largely used, forming an exception to a law of the disease also ascertained by him,—such observations, I say, open to us large vistas of thought. In truth, the mere advance from speculations on the quantity of circulating blood to its crisis is a very important step in the right direction. John Hunter

felt the want of inquiries pushed into that region, but does not appear to have clearly seen the medium through which they should be made. This, at least, I must presume from his mode of adverting to the subject. "The mode of examining the blood, when out of the body, enables us," he says, "to observe whatever relates to its spontaneous changes and separation, together with the apparent properties of each component part. Its chemical properties become known likewise by this mode, though without throwing much light on the nature of the fluid itself." The great intellect of John Hunter had no doubt conceived profound thoughts respecting the *nature* of the blood. After all, however, we must be at present contented with ascertaining its physical *laws*. And it is improbable that more truth will be realized respecting it by any line of research than is made visible in the distance through the operations of chemistry.

The use of remedies is an element of medical inquiry in which our reasoning both by observation and experiment is of the highest importance. It is the *champ-de-bataille* of empirics, who take refuge in the vagueness which it admits, from the more stringent and exact subjects of pathology. By ourselves it is often treated with an appearance of indifference, as compared with pathological inquiry, which is not justified either by the amount of knowledge attainable, or its usefulness when attained. But the tendency to depreciate remedies, or to delight in finding out that they are convertible, and that a disorder gets well just as readily under one treatment as under another, belongs to second-rate

minds. To persons who lean in this direction, I beg to suggest the perusal of the following case:—"A lady," says John Hunter, "of what is called a nervous constitution, arising in some degree from an irritable stomach, often troubled with flatulencies, and what are called nervous head-aches, with pale urine, at these times uncomfortable feelings, and often sinkings, had a tumour removed from the breast, and likewise from near the arm-pit. Nothing appeared uncomfortable for a few days, when very considerable disorders came on. She was attacked with a shivering or cold fit, attended with a feeling of dying, and followed by a cold sweat. It being supposed she was dying, brandy was thrown in, which soon brought on a warmth, and she was relieved: the fits came on frequently for several days, which were always relieved by brandy; and she took in one of the most violent of them about half a pint. While under these affections she took the bark as a strengthener, the musk, occasionally, as a sedative, in large quantities, camphorated jalap frequently, and, towards the last, valerian in large quantities. But whatever effect these might have in lessening the disease on the whole, they certainly were not equal to it without the brandy. Brandy removed these dying fits; and I thought they became less violent after taking the valerian. A question naturally arises, would the brandy alone, if it had been continued as a medicine, have cured her without the aid of the other medicines? The other medicines, I think, certainly could not have done it; nor do I think the brandy could have been continued in such large quantities as to have prevented

their returns. If so, the two methods were happily united; the one gradually to prevent, the other to remove immediately the fits when they came on.”* Now, the main object of this case, as adduced by John Hunter, was pathological; while the carefulness with which he analyses the treatment, illustrates his opinion of its relative importance as a subject of observation. As contrasted with the above, I may refer my reader to a very large proportion of the valuable cases in Dr. Abererombie’s works on Diseases of the Brain and Spinal Cord, and on Diseases of the Stomach and Abdominal Viscera, wherein treatment is so slightly and generally given, that a question repeatedly arises, whether the phenomena really illustrate the laws of the disease under which they are described, or the effects of remedies used, or in what degree they illustrate either.

I am disposed to think that our pathological inquiries might, in some cases, be advantageously commenced from the therapeutical side of the subject; and the order “*dato morbo invenire remedium*,” might so far be reversed. The question, what shall I do for the removal of this group of symptoms? would often receive an answer virtually assigning to the group its nosological place and name. The truths which had escaped us in speculation, often come vividly before us when the mind is thrown into a practical condition. This method of dealing with a difficulty is of course not meant for application to the neglect of the more

* Hunter on the Blood, Inflammation, &c.

scientific procedure; but merely in cases in which the latter has failed to convey light.

With respect to the kind of proof applicable to the discovery and use of remedial agents, in the first point of view it admits of experiment; in the second point of view it depends upon observation alone. A confusion between the analytical experiments which discover, and the observations founded on experience which apply, will be full of mischief. The question, whether quina in its combinations is convertible with cinchona bark, was at first often begged, through this oversight, very much to the disadvantage of the public. Morphia, in its relations to opium, is similarly circumstanced: in both cases the experimental procedure of the chemist must be made subservient to the experience of the physician.

As I am discussing the subject of medical proof, it may be allowable to call my readers' attention to the necessity which exists that this, in its therapeutical point of view, should be contemplated in regard to some methods of practice, opened to us, or at least greatly extended, within the last thirty years. Our communication with Europe, and through Europe, is such, that remedies local to other countries become a part of the subject with which our practitioners are justly expected to be conversant. On these grounds I need scarcely suggest the expediency of our placing ourselves in a more complete acquaintance with the continental spas in their application to English habits and constitutions. Knowledge on these subjects is at present almost confined to the physician practising at the re-

spective spas, instead of being a part of the great system of European medicine. The instances in which benefit is undoubtedly derived from a very copious introduction of diluted neutral salts with gaseous impregnations into the system, and the cases in which this is prejudicial, *e. g.* the effect for good or evil of the bath-sturm, as the physicians of Carlsbad term that pyretic state which they induce, with its resulting phlegmonous abscesses ;—these are weighty considerations, as yet if not unexplored, certainly not exhausted. At present a patient is sent precariously to a spa by one who knows nothing about *it*, and there he falls into the hands of some one else who knows nothing about *him*. Of course I put this down as a liability, not as a necessary fact.

The subject matter of medical reasoning, which I have hitherto contemplated, has been either entirely physical or viewed in physical relations, having, as such, a close connexion with mental phenomena, since the brain is the organ of mind. But we have a philosophical right to contemplate mind in a certain primary sense, and as possessing properties on which we may reason as such, just as we reason on extension and solidity as properties of the cerebral substance. Though we should accept Dr. Faraday's revival of Boscovich's theory, and resolve the material world into centres of power, we shall still be met by this distinction, and compelled to admit* its expediency; for at all events the domains of thought and sensation are so placed with respect to each other, that each will be practically best estimated when their distinction is kept in mind.

Thus a disease may have its moral or physical phase, according to the position from which we contemplate it.

The question first to be considered in reference to my present inquiry is, whether the proof applicable to the phenomena of mind, and its laws in health and disease, be that of experiment or mere observation? And that question may, I believe, be decided in favour of the latter source of proof, as having most to do with this subject. In a passage, which I readily quote, as bearing upon the general distinction between observation and experiment, a distinguished writer in the *Edinburgh Review* remarks: "By experiment we generally acquire a pretty correct knowledge of the causes of the phenomena which we produce, as we ourselves distribute and arrange the circumstances on which they depend; while in matters of mere observation the assignment of causes must always be in a great degree conjectural, inasmuch as we have no means of separating the preceding phenomena, or deciding, otherwise than by analogy, to which of them the succeeding event is to be attributed."

"Now it appears to us," the reviewer proceeds, "to be pretty evident that the phenomena of the human mind are almost all of the latter description."*

This distinction does not indeed imply that the properties of the human mind are placed out of our reach, that we have no power through education or other moral management of controlling, of mitigating, or of antagonising one property by exciting the action of

* *Edinburgh Review*, vol. iii., p. 275.

another. I am, however, induced to dwell upon the distinction, by the tendency of philosophers to overlook it, and to reason upon these properties as if they were the subject of experiment, as if we could practise upon them the "*separatio naturæ*," which discovers causes, instead of contenting ourselves with the process of observation, which develops laws. Much of the indefiniteness of the science of mind has arisen from this appetency of a knowledge beyond our present means of attainment. If any one doubt this, I advise him to peruse Dr. Wigan's ingenious work, in which he professes to prove "the duality of the mind." The knowledge that we have two brains has long been in the possession of the physiologists, and the subject has been variously discussed by them: that Gall knew one brain might be insane, the other healthy, has been pointed out by Dr. Elliotson. Tiedemann relates the case of a man who had one side of the brain deranged, and who observed its derangement with the healthy side. Bichat has some curious remarks on this subject:—"If one of the hemispheres," he observes, "is better organised than the other, more developed in all its points, consequently capable of being more strongly affected, then I maintain that perception will be confused. . . . Therefore, if we could squint with this organ as we can with the eyes, that is, receive impressions with but one hemisphere, we should then be masters of the accuracy of our intellectual exertions." Now, these physiologists have made good, by observation, the capacity of the mind, whether double or single, to energise with one brain, under defect or de-

struction of the other ; and they conjecture its liability to suffer impairment in its operations from a want of harmonious action of the two brains ; but they do not assert the ulterior fact that we have two *minds*, because they are logically aware that the nature of their proof does not reach this proposition, which demands a “*separatio naturæ*,” a power of witnessing the two minds in a state of distinctness, neither at present cognizable to us, nor, indeed, conceivable. No such proof is adduced by Dr. Wigan ; and all the proof that he does advance is explainable on the hypothesis of a single mind, viewed in relation either to antagonism of faculties, or to want of harmony in the two brains, or to disease of one of them.

Our acquaintance with the normal phenomena of mind must precede our acquaintance with the abnormal. This is sufficiently obvious ; yet the truth is practically less felt in mental than in physical disease. To Reid and Stewart we are indebted more than to any other philosophers, not also physiologists, for that modest course of observation which may lead to the establishment of laws in the science of mind. So far, indeed, as the inquiry can proceed advantageously without the assistance derivable from a consideration of structure, they have done well.

No eminent philosopher, with the exception perhaps of M. Comte, has given the deserved credit to the views of the phrenologists in this direction. Every error of diagnosis that they have committed has been brought to bear upon them by their adversaries, as if subversive of their system : no candid inquirer has sug-

gested on this point that their proof being drawn from quantity and form of the cerebral substance, irrespectively of quality, would be truly suspicious if it seemed to be uniformly successful. It must be confessed, however, that the phrenologists have been incautious in their management of the startling part of their system, namely, their map of the eranium. They have never, as far as I can find, recorded a distinct admission that this same map is a purely speculative hypothesis, intended to be such an approximation to the results of their observations, as to afford aid in applying them to practice. To assert categorically, for example, that the properties of hope and conscience are separated from each other by a straight line, inclining or slanted from the occiput to the forehead, and without some such modification, is an absurdity. And this absurdity is repeated in kind through every page of their history of organs. As an instrument for carrying out their researches, their chart of the brain is judicious enough; viewed as a “*fait accompli*,” a discovery, it is laughable; and more than anything else has retarded their attainment of their just rank among the philosophers of mind.

When observations shall have gone farther towards the development of the laws of the sound mind, we shall become more fitted to appreciate its unsound states. But there are many difficulties created by the feelings and sympathies of our nature, independently of its reasonableness, which will evermore beset this subject. There will evermore be an unwillingness to admit the proof of the existence of mental unsoundness, where this proof, if admitted, will control the

liberty or the fortunes of the patient. There will be an equal unwillingness to admit the absence of such unsoundness, when the imputation of it would shelter him as a delinquent from punishment. These difficulties, which have a basis in the very constitution of the human mind, are so immediately and practically felt in the present day, that some remedy is at once desirable, if it can be afforded.

I have in other works endeavoured to make good a distinction, which might be carried into effect with advantage in both these points, but principally in enabling us to appreciate a form of *moral* unsoundness, which need not be contemplated as exculpatory where it is discovered in a criminal.* If any of my readers should be in doubt whether this subject requires a sounder discretion and more comprehensive views than are established at present, I beg to refer them to a case recently tried before Mr. Justice Maule, in which it was decided that instead of punishment, a comfortable abode for life should be provided for a tradesman, who under normal feelings of resentment and hatred, labouring under no incoherency of language or manner, either at the time or afterwards, had shot through the head another tradesman, against whom he entertained a grudge.

In cases of the above kind, it may be observed, that the medical witnesses brought into court are generally the *prisoner's* witnesses. They are provided by his solicitor, who, of course, will only bring forward such as are able conscientiously to enforce the plea of

* Elements of Pathology of the Mind; Clinical Facts and Reflections.

insanity. The interests of society remain unprotected, so far as that profession is concerned by which the jury is presumed to be instructed of the state of the delinquent's mental soundness. Surely, a medical assessor is required in such cases, who might, in some degree, neutralise this evil.

From this brief sketch of the materials of medical reasoning as viewed in relation to its modes and principles, I now return to some further consideration of these modes and principles ; a consideration which I have purposely left for this place, as it may best be illustrated from the materials with which the reasoning deals.

I have ventured to affirm, that observation, as distinguished from experiment, is the main source of medical proof ; and I have endeavoured at the same to point out the extent to which experiment may be subservient or auxiliary to observation. Consistently with these views, the following three principles may be laid down as necessary to the completeness of an inductive process through observation :—

1st.—That the observations, on which the induction proceeds, should possess truly common points.

2ndly.—That these observations should be adduced in sufficient number : this sufficiency involving a larger or a smaller number, according to the absence or presence or amount of antecedent probability, on which the induction proceeds.

3rdly.—That the hypothesis, containing such grounds of antecedent probability, should itself be well substantiated ; and also that its connection with the observations which are referred to it, should be well substantiated.

The importance of the first of these rules requires no commentary. The main element of the second rule is scarcely less obvious. Thus, in the summer of 1846, the inhalation of ether was brought forward as a means of safely preventing sensation, by inducing a form of stupefaction or asphyxia. Here was a suggestion to be made good or nullified by observed facts. But it was of great antecedent improbability; and the caution of our surgeons in not giving it their entire assent, until accumulated instances had neutralised the improbability, has been proportionate. The recently-discovered agent, chloroform, is differently situated. Antecedent improbability is in a great degree removed by the acknowledged success of another similar agent, and *its* claims may be admitted on a more limited induction. With respect to the third rule, I have already offered such arguments as occur to me in favour of its first clause, namely, that the hypothesis from which a process of inductive observation sets out, should itself have been well substantiated; and its second clause, that the connexion between the inductive process and this hypothesis should be also well substantiated, will, perhaps, appear important, when some of its infractions shall have been adduced.

A forced accommodation of an inductive truth, as an hypothesis to a series of observations, is one of those errors into which a philosophical tendency to generalise is apt to seduce us, and I think I have observed instances in which the philosopher has indemnified himself by almost heedless precipitancy in this work of application, for the pains which he had taken in establishing the truth of the hypothesis itself. The following

remarks, from the valuable work of Dr. Owen Rees, indicate the defective logic in this kind, to which the science of chemistry is exposed in its relation to medicine. After some observations on the oxalate of lime deposit, he proceeds: "Further and more correct observations are needed into the physical conditions of patients suffering from oxalate of lime deposits, than any we yet possess. We find that chemistry is at no loss, however, to devise theories for the transformation of several organic principles into oxalic acid, and whether it be derived from sugar, urea, or lithic acid, we can make our formula by adding or abstracting oxygen, as the case may require. All this, however, must be looked upon as a display of ingenuity on the part of the chemist; and we should wait till accurate and long continued observations, conducted on the urine of patients, help to better evidence on which to form a conclusion. Unfortunately, the addition or subtraction of oxygen, necessary to some of these theories, has not been proved, or even rendered probable, and no good reason has been given in most cases for transferring one proximate element more than another for the formation of a diseased product. It is often the case, that more than one proximate element would answer the purpose, owing to the similarity of composition. The profusion with which the chemists are in the habit of adding or taking away oxygen by as many atoms as it may please them, still further lessens the difficulty that may at first appear to stand in the way of effecting an explanation." *

* Dr. Owen Rees on Urinary Diseases, page 147.

These admirable remarks on the theory of metamorphosis in the production of oxalic acid, suggest the two sources of primary and secondary assimilation from which it may be derived. Nor, it must be confessed, is it unreasonable, if imperfect assimilation in the primary digestive organs be the general source of it, that this should be made a general ground of treatment. But if the supply of oxalic acid may be as large or much larger, through imperfect combinations with oxygen in the secondary assimilation, then a general proscription of sugar, in all cases of oxalate of lime deposits, becomes unphilosophical, prior to a diagnosis being obtained between these two sources in relation to the presence of oxalic acid; that is to say, it becomes amenable to Dr. Owen Rees's objection against those chemists who make an arbitrary choice of one proximate element rather than another, for the formation of a diseased product. Now we are, in fact, made aware of a much more copious source of oxalic acid, by M. Liebig's remark, that it may be produced from uric acid, whenever it is subjected to the imperfect action of oxygen; and Dr. Aldridge, of Dublin, has shewn that lithic acid, by the addition of the elements of water in varying proportions, may be theoretically converted into oxalate and carbonate of ammonia, hydrocyanic and formic acids, according to the circumstances of decomposition. Under this *embarras de richesses*, supplied by the secondary as well as the primary assimilations in the formation and deposit of this morbid product, chemistry is misapplied if it lead to the assumption of one cause, and that cause theoretically the least

adequate one, as a basis of treatment, without the most explicit admission, that this assumption is partial and imperfect. At all events, the *empirical* question of diet has a right to consideration under these or analogous circumstances, before a change is made.*

Speaking of the progress of his science, M. Liebig observes : “ It is universally felt, that we are as far from a true animal chemistry, as the anatomy of the last century was from the physiology of the present day.” This sentence may be true ; but it is gratifying to collect from it a prophetic anticipation of an improvement of animal chemistry analogous to that of the science with which he contrasts it. With a view to the practical results of this scientific improvement, it is of incalculable importance that the deductive logic of chemistry should be as cautious as its inductive.

Instances, however, of the departure from sound logic, which I am illustrating, may be found even in M. Liebig. The following example may be considered in point. By experiments proving that “ animals cooled rapidly, notwithstanding the blood appeared to undergo the usual changes in the lungs,” Mr. Brodie had disproved the opinion, then generally received, that animal heat is dependent in warm-blooded animals on the changes produced in the blood by respiration. M. Liebig’s experiments proceeded to remove entirely the causation of animal heat from combination of inspired oxygen with carbon contained in the blood, and to place it in the metamorphosis through combus-

* See Dr. Prout on Stomach and Urinary Disease.

tion with oxygen of living tissues formed from the blood. It is next argued by him, that in the healthy subject a quantity of carbon must be introduced as food into the system, corresponding with the quantity of oxygen introduced. But the quantity of oxygen inspired in a given volume of air is affected, he observes, by the temperature and corresponding density of the atmosphere. Now, in applying these important views as an hypothesis to physiological observation, he instantly quits his inductive circumspection. Assuming the capacity of the chest to be a "constant quantity,"* he argues that at every inspiration an amount of air enters, the volume of which may be considered as uniform." But its density, and consequently the quantity of oxygen which a given volume contains, is not constant. "Air is expanded by heat, and contracted by cold, and therefore equal volumes of hot and cold air contain unequal weights of oxygen. In summer, moreover, atmospheric air contains aqueous vapour, while in winter it is dry; the space occupied by vapour in the warm air is filled up by air itself in the winter." On this account, also, "atmospheric air contains for the same volume more oxygen in winter than in summer." Meanwhile, "in summer and winter at the equator and the poles we respire an equal volume of air."

From these data, "the oxygen taken into the system by inspiration being given out in the same form in summer and winter," M. Liebig infers that we expire

* Liebig's Organic Chemistry of Physiology, pp. 16-17. I am not concerned with M. Liebig's facts, but merely with his inferences.

more carbon in cold weather, and when the thermometer is high, than we do in warm weather, and “we must consume more carbon in our food in the same proportion in Sweden than in Sicily, and in our own more temperate climate a full eighth more in winter than in summer.”

Now, as a comment on this dietetic generalisation, it may be observed, that the volume of inspired air is not *necessarily* uniform, supposing it granted that the capacity of the chest is a constant quantity. This requires further proof. What is the phenomenon of gasping for breath, when a high elevation may have been obtained, but an endeavour to take in a larger volume of air, probably in order to compensate for the diminished amount of oxygen in a given volume of it? I have a right to suppose, previous to disproof, that this is only an exaggerated degree of a process of accommodation, which may be constantly taking place on a smaller scale. Whence comes that choice of farinaceous food, which the exigencies of his nature seem to dictate to the Hindoo, if his warm climate make a smaller quantity of carbon necessary to him in his food than to the inhabitant of a colder climate, and equal volumes of air are always inspired by each, agreeably to M. Liebig?

I am drawn into these remarks incidentally, and with no disposition to criticise great philosophers farther than is requisite for the illustration of a logical error. It must, however, be admitted, that the immense physiological consequences arrived at through the last precipitate deduction, give a practical in-

terest to my remarks. M. Liebig himself deals somewhat severely with physicians in *their* specific character of pathologists. "All the new facts daily ascertained by the chemists are," he says, "regarded by the pathologists as exactly those which are of no use to them, because they have no clear idea of what they require; because they are unable to connect with these chemical discoveries any questions to be solved, or to draw from them any conclusion*." *If* they are unable to draw conclusions from these discoveries, the above remark at least assumes that they are conscious of their inability, and are thus guaranteed against precipitate generalisations. Let M. Liebig remember, that it is far better to ponder over great inductive truths, even for many years, and deliberately to reduce them into practice, than to spoil their application, and to prejudice the public by precarious deductions from them.

In the above outline of medical proof, I have supposed my subject to begin at the point at which Mr. Green closes his admirable oration. He has there set forth the state of mind into which the student should be brought, antecedently to his direct study of medicine; and of which it may be truly said, that it is the fitting basis of a professional education in the largest sense of that term. The views which I have endeavoured to open, as they more immediately concern *medical* education, no doubt presuppose some acquaintance with its subject matter. I could not have explained the instruments of our labours without alluding

* Researches on the Chemistry of Food, page 3.

to the materials on which they have to operate. It may be observed, that many of my illustrations of successful or valuable reasoning have been drawn from the works of physicians, who are either the authors or the offspring of our present system of medical education. Those who are disposed to peril that system, by subjecting its future regulation to an unprofessional tribunal, would do well to satisfy themselves by further inquiry as to the nature of that education, and the examinations by which we test it. The inquiry may lead them to question the expediency of sacrificing us to an act of the legislature, which assumes that the best efforts of our profession are valueless in respect to the management of our studies. The purport of the late Registration Bill is to place us in these and all other respects at the disposal of a Board, not one member of which would be necessarily or *ex officio* a medical practitioner!

It may be collected from the above outline of medical proofs that we do not obtain it from positive data. It is drawn neither from precedents, nor from custom, nor from authority, but from the arcana of nature. This is at once our prerogative, and the main source of our difficulty. While deductive reasoning forms the substance of the logic of the other professions, it is in medicine only an element, and a subordinate one, in the intellectual process. Hence arises to us another peculiarity. Under the hindrances which beset us in the acquisition of principles, or the danger of giving them a forced adaptation, we must often be content to act empirically, and to rely upon the *tact* which may

be presumed to result from experience, even where it has not led to inductive conclusions. Meanwhile to discriminate the instances and the extent in which we may substitute this subtle quality for deductions from observation, constitutes a trial of the judgment, to which, I believe, no other pursuit is subjected in an equal degree. And, while I endeavour to facilitate the student's progress through the trial, I must remind him, that the manly preparation, both of the intellects and the heart, which he may learn from the oration of Mr. Green, and yet more from the writings of Lord Bacon, will best enable him to encounter it.

A P P E N D I X.

NOTE 1 (p. 22.)

OF this probable indefiniteness, and the mischief connected with it, a glaring instance is afforded in the arrangements made for obtaining Statistical Reports under the present Sanitary Commission.

In one great metropolitan workhouse, two medical officers receive nine printed forms, under the various heads of which they are requested to furnish all the details connected with this important subject, in a very large district. They are requested to execute this work as quickly as possible, under the immediate

expectation of Asiatic Cholera. This work forms no part of their stipulated and paid parochial occupation. They may complete it carelessly: indeed, they have not time, from their other occupations, to complete it well; or they may decline to complete it. In the latter case they are liable to be told by the Board of Guardians, under whom they are appointed, on a complaint being made by the Commissioners, that they *must* undertake this supplementary labour: in other words, that the numerous paupers whom they were engaged to attend, must be inadequately tended, and that the Statistical Report must be imperfectly made out.—If both these duties are thrown upon them, both must be neglected.

When will the public have found out, that justice towards *this* class of officers is justice towards themselves? Let it be remembered, that these gentlemen form a large proportion of the young practitioners of the country: that if, in consequence of being thus loaded with work at once excessive and unremunerated, they become habituated to imperfect performance of it, two important results must ensue; one of them—the direct result,—that important duties will be ill discharged; the other—the indirect result,—that a School of Medicine (of the utmost possible im-

portance) will be rendered worse than useless, by its ministering to habits of desultory practice. The appeal to the sympathies of the public, in favour of this class of practitioners, has been made in vain.—I am anxious to address myself to its selfishness.

NOTE 2 (p. 38.)

A more complete view than I profess to give of the subject of medical proof, would contain a large discussion of this element. In reference to the remarks in the text, I may observe, that, in doubtful cases, we ought to be very sure of our scientific law or explanation before we allow it to contravene our empirical instincts. A man of a cultivated mind—and of such I speak—is, in fact, often less empirical than he may suppose himself. Conjectures, which appear to him mainly of this nature, are not wholly so. A forgotten process of reasoning has often left in his mind the idea which afterwards occurs irrespectively of this process, and he may neglect the conclusions of his judgment if he at once rejects what assumes an unphilosophical aspect, as not being connected with any *known* process of reasoning. Empirical practice has often proved infinitely less delusive than imperfect science; and has

itself been justified by more complete investigation. Thus, the use of alkalies, in many cases of an alkaline condition of the urine, had been long empirically found palliative; it is *primâ facie* condemned by chemical reasoning: it has been finally placed on a scientific basis by the suggestion of Dr. Owen Rees,* that in these cases the abnormal state of the urine results from a condition of “the lining membrane of the urinary apparatus,” which alkaline remedies will remove.

* Opus citat. p. 137.

THE END.

